Stack
ACK ANN
500
5771
A
000084135
3
UC SOUTHERN REGIONAL LIBRARY FACILITY

MR. HERBERT SPENCER

AND THE

BRITISH QUARTERLY REVIEW.

NOTE.

A PASSAGE from the *Treatise on Natural Philosophy*, written by the two Professors of Physics in the Universities of Glasgow and Edinburgh, has been quoted by a writer in the *British Quarterly Review* to disprove a position of mine respecting certain primary scientific truths. As the question concerning our warrant for physical axioms, will, by most, be classed as a question in Physics, instead of being classed as a question in Logic and Psychology, as it should be, most will assume that the judgment of Prof. Tait endorsed by one who ranks so high as Sir William Thomson, is decisive. Partly because the conclusion controverted is one to which I have committed myself, and partly because theories respecting the bases of scientific knowledge have both a present interest and a permanent importance, there seems occasion for a reply.

It is, indeed, probable that though the names arrayed against me are weighty, and though the writer of the review, known in scientific circles as the Senior Wrangler of his year, may be deemed specially competent as a critic, I should have taken no immediate notice of the attack made by him, had it not been for passing circumstances. At the time when the *British Quarterly Review* containing his article appeared, I was preparing for the *Fortnightly Review*, certain "Replies to Criticisms;" dealing with objections to doctrines of mine, chiefly metaphysical, that have been made by various writers during the last ten years. Having undertaken this task, I could not pass over this further criticism without causing misconstruction. I therefore devoted to it such few pages as were available: dealing chiefly with the impersonal question raised, and very briefly with the allegations which touched me personally.

Here the matter would have rested had not the reviewer returned to the attack in the January number of the *British Quarterly:* taking advantage of my silence respecting charges I had no space to meet. As I was about to include these "Replies to Criticisms" in a forthcoming volume of Essays, I decided to expand my answer to him; partly that I might justify more fully the general doctrine controverted, and partly that I might rebut his mis-statements. Having done this, I have thought it well to print separately a few copies of the expanded answer, for distribution among those who may possibly be interested.

LONDON, *Feb.*, 1874.

Who is Senior Wrangler for 1868?

Stack
Annex
00 ENG
500
5771

MR. HERBERT SPENCER

AND THE

BRITISH QUARTERLY REVIEW.

NOTICE.

Mr. Spencer regrets that since the completion of "The Study of Sociology," *sundry minor matters have prevented him from resuming his serial. The first number of* "The Principles of Sociology," *is, however, partly written; and he hopes to issue it in May.*

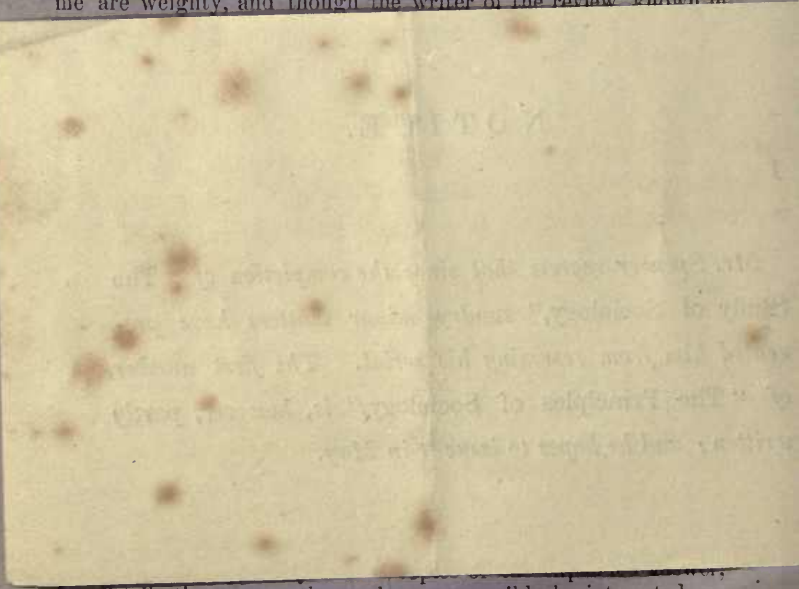
in the statement of physical principles and for erroneous reasoning in physics. That he discovers no mistakes I

5005771

NOTE.

A PASSAGE from the *Treatise on Natural Philosophy*, written by the two Professors of Physics in the Universities of Glasgow and Edinburgh, has been quoted by a writer in the *British Quarterly Review* to disprove a position of mine respecting certain primary scientific truths. As the question concerning our warrant for physical axioms, will, by most, be classed as a question in Physics, instead of being classed as a question in Logic and Psychology, as it should be, most will assume that the judgment of Prof. Tait endorsed by one who ranks so high as Sir William Thomson, is decisive. Partly because the conclusion controverted is one to which I have committed myself, and partly because theories respecting the bases of scientific knowledge have both a present interest and a permanent importance, there seems occasion for a reply.

It is, indeed, probable that though the names arrayed against me are weighty, and though the writer of the review, known in



for distribution among those who may possibly be interested.

LONDON, *Feb.*, 1874.

Who is senior wrangler for 1868?

Stack
Annex
00 ENG
500
5771

MR. HERBERT SPENCER

AND THE

BRITISH QUARTERLY REVIEW.

[*The first eleven of the following pages have already appeared at the close of the "Replies to Criticisms," in the* Fortnightly Review. *I reproduce them here because, in their absence, the additional portion would not be wholly intelligible.*]

My excuse for devoting some space to a criticism of so entirely different a kind as that contained in the *British Quarterly Review* for October, must be that, under the circumstances, I cannot let it pass unnoticed without seeming to admit its validity.

Saying that my books should be dealt with by specialists, and tacitly announcing himself as an expert in Physics, the reviewer takes me to task both for errors in the statement of physical principles and for erroneous reasoning in physics. That he discovers no mistakes I

500[illegible]

do not say. It would be marvellous if in such a multitude of propositions, averaging a dozen per page, I had made all criticism-proof. Some are inadvertencies which I should have been obliged to the reviewer for pointing out as such, but which he prefers to instance as proving my ignorance. In other cases, taking advantage of an imperfection of statement, he proceeds to instruct me about matters which either the context, or passages in the same volume, show to be quite familiar to me. Here is a sample of his criticisms belonging to this class :—

"Nor should we counsel a man to venture upon physical speculations who converts the proposition '*heat is insensible motion*' into '*insensible motion is heat*,' and hence concludes that when a force is applied to a mass so large that no motion is seen to result from it, or when, as in the case of sound, motion gets so dispersed that it becomes insensible, it turns to heat."

Respecting the first of the two statements contained in this sentence, I will observe that the reader, if not misled by the quotation-marks into the supposition that I have made, in so many words, the assertion that "insensible motion is heat," will at any rate infer that this assertion is distinctly involved in the passage named. And he will infer that the reviewer would never have charged me with such an absurd belief, if there was before him evidence proving that I have no such belief. What will the reader say, then, when he learns, not simply that there is no such statement, and not simply that on the page referred to, which I have ascertained to be the one intended, there is no such implication visible, even to an expert (and I have put the question to one); but when he further learns that in other passages, the fact that heat is one only of the modes of insensible motion is distinctly stated (see *First Prin.* §§ 66, 68, 171); and when he learns that elsewhere I have specified the several forms of insensible motion? If the re-

viewer, who looks so diligently for flaws as to search an essay in a volume he is not reviewing to find one term of an incongruity, had sought with equal diligence to learn what I thought about insensible motion, he would have found in the *Classification of the Sciences*, Table II., that insensible motion is described by me as having the forms of Heat, Light, Electricity, Magnetism. Even had there been in the place he names, an unquestionable implication of the belief which he ascribes to me, fairness might have led him to regard it as an oversight when he found it at variance with statements I have elsewhere made. What then is to be thought of him when, in the place named, no such belief is manifest; either to an ordinary reader or to a specially-instructed reader?

No less significant is the state of mind betrayed in the second clause of the reviewer's sentence. By representing me as saying that when the motion constituting sound "gets so dispersed that it becomes insensible, it turns to heat," does he intend to represent me as thinking that when sound-undulations become too weak to be audible, they become heat-undulations? If so, I reply that the passage he refers to has no such meaning. Does he then allege that some part of the force diffused in sound-waves is expended in generating electricity, by the friction of heterogeneous substances (which, however, eventually lapses from this special form of molecular motion in that general form constituting heat); and that I ought to have thus qualified my statement? If so, he would have had me commit a piece of scientific pedantry hindering the argument. If he does not mean either of these things, what does he mean? Does he contest the truth of the hypothesis which enabled Laplace to correct Newton's estimate of the velocity of sound—the hypothesis that heat is evolved by the com-

pression each sound-wave produces in the air? Does he deny that the heat so generated is at the expense of so much wave-motion lost? Does he question the inference that some of the motion embodied in each wave is from instant to instant dissipated, partly in this way and partly in the heat evolved by fluid friction? Can he show any reason for doubting that when the sound-waves have become too feeble to affect our senses, their motion still continues to undergo this transformation and diminution until it is all lost? If not, why does he implicitly deny that the molar motion constituting sound, eventually disappears in producing the molecular motion constituting heat?*

I will dwell no longer on the exclusively-personal questions raised by the reviewer's statements; but, leaving the reader to judge of the rest of my "stupendous mistakes" by the one I have dealt with, I will turn to a

* Only after the foregoing paragraphs were written, did the remark of a distinguished friend show me how certain words were misconstrued by the reviewer in a way that had never occurred to me as possible. In the passage referred to, I have said that sound-waves "finally die away in generating thermal undulations that radiate into space;" meaning, of course, that the force embodied in the sound-waves is finally *exhausted* in generating thermal undulations. In common speech, the dying-away of a prolonged sound, as that of a church-bell, includes its gradual diminution as well as its final cessation. But rather than suppose I gave to the words this ordinary meaning, the reviewer supposes me to believe, not simply that the *longitudinal* waves of air can pass, *without discontinuity*, into the *transverse* waves of ether, but he also debits me with the belief that the one order of waves, having lengths measurable in feet, and rates expressed in hundreds per second, can, *by mere enfeeblement*, pass into the other order of waves, having lengths of some fifty thousand to the inch, and rates expressed in many billions per second! Why he preferred so to interpret my words, and that, too, in the face of contrary implications elsewhere (instance § 100), will, however, be manifest to every one who reads his criticisms.

question worthy to occupy some space, as having an impersonal interest—the question, namely, respecting the nature of the warrant we have for asserting ultimate physical truths. The contempt which, as a physicist, the reviewer expresses for the metaphysical exploration of physical ideas, I will pass over with the remark that every physical question, probed to the bottom, opens into a metaphysical one; and that I should have thought the controversy now going on among chemists, respecting the legitimacy of the atomic hypothesis, might have shown him as much. On his erroneous statement that I use the phrase "Persistence of Force" as an equivalent for the now-generally-accepted phrase "Conservation of Energy," I will observe only that, had he not been in so great a hurry to find inconsistencies, he would have seen why, for the purposes of my argument, I intentionally use the word Force: Force being the generic word, including both that species known as Energy, and that species by which Matter occupies space and maintains its integrity—a species which, whatever may be its relation to Energy, and however clearly recognized as a necessary *datum* by the theory of Energy, is not otherwise considered in that theory. I will confine myself to the proposition, disputed at great length by the reviewer, that our cognition of the Persistence of Force is *à priori*. He relies much on the authority of Professor Tait, whom he twice quotes to the effect that—

"Natural philosophy is an experimental, and not an intuitive science. No *à priori* reasoning can conduct us demonstratively to a single physical truth."

Were I to take a hypercritical attitude, I might dwell on the fact that Professor Tait leaves the extent of his proposition somewhat doubtful, by speaking of "Natural philosophy" as *one* science. Were I to follow further

the reviewer's example, I might point out that "Natural philosophy," in that Newtonian acceptation adopted by Professor Tait, includes Astronomy; and, going on to ask what astronomical "experiments" those are which conduct us to astronomical truths, I might then "counsel" the reviewer not to depend on the authority of one who (to use the reviewer's polite language) "blunders" by confounding experiment and observation. I will not, however, thus infer from Professor Tait's imperfection of statement that he is unaware of the difference between the two; and shall rate his authority as of no less value than I should, had he been more accurate in his expression. Respecting that authority I shall simply remark that, if the question had to be settled by the authority of any physicist, the authority of Mayer, who is diametrically opposed to Prof. Tait on this point, and who has been specially honoured, both by the Royal Society and by the French Institute, might well counterweigh his, if not out-weigh it. I am not aware, however, that the question is one in Physics. It seems to me a question respecting the nature of proof. And, without doubting Professor Tait's competence in Logic and Psychology, I should decline to abide by his judgment on such a question, even were there no opposite judgment given by a physicist, certainly of not less eminence.

Authority aside, however, let us discuss the matter on its merits. In the *Treatise on Natural Philosophy*, by Professors Thomson and Tait, § 243, I read that "as we shall show in our chapter on 'Experience,' physical axioms are axiomatic to those only who have sufficient knowledge of the action of physical causes to enable them to see at once their necessary truth." In this I agree entirely. It is in Physics, as it is in Mathematics, that before necessary truths can be grasped, there must

be gained by individual experience, such familiarity with the elements of the thoughts to be framed, that propositions about those elements may be mentally represented with distinctness. Tell a child that things which are equal to the same thing are equal to one another, and the child, lacking a sufficiently-abstract notion of equality, and lacking, too, the needful practice in comparing relations, will fail to grasp the axiom. Similarly, a rustic, never having thought much about forces and their results, cannot form a definite conception answering to the axiom that action and reaction are equal and opposite. In the last case as in the first, ideas of the terms and their relations require to be made, by practice in thinking, so vivid that the involved truths may be mentally seen. But when the individual experiences have been multiplied enough to produce distinctness in the representations of the elements dealt with; then, in the one case as in the other, those mental forms generated by ancestral experiences, cannot be occupied by the elements of one of these ultimate truths without perception of its necessity. If Professor Tait does not admit this, what does he mean by speaking of "physical *axioms*," and by saying that the cultured are enabled "to see *at once* their *necessary* truth?"

Again, if there are no physical truths which must be classed as *à priori*, I ask why Professor Tait joins Sir W. Thomson in accepting as bases for Physics, Newton's Laws of Motion? Though Newton gives illustrations of prolonged motion in bodies that are little resisted, he gives no *proof* that a body in motion will continue moving, if uninterfered with, in the same direction at the same velocity; nor, on turning to the enunciation of this law quoted in the above-named work, do I find that Professor Tait does more than exemplify it by facts

which can themselves be asserted only by taking the law for granted. Does Professor Tait deny that the first law of motion is a physical truth? If so, what does he call it? Does he admit it to be a physical truth, and, denying that it is *à priori*, assert that it is established *à posteriori*—that is, by conscious induction from observation and experiment? If so, what is the inductive reasoning which can establish it? Let us glance at the several conceivable arguments which we must suppose him to rely on.

A body set in motion soon ceases to move if it encounters much friction, or much resistance from the bodies struck. If less of its energy is expended in moving, or otherwise affecting, other bodies, or in overcoming friction, its motion continues longer. And it continues longest when, as over smooth ice, it meets with the smallest amount of obstruction. May we then, proceeding by the method of concomitant variations, infer that were it wholly unobstructed its motion would continue undiminished? If so, we assume that the diminution of its motion observed in experience, is proportionate to the amount of energy abstracted from it in producing other motion, either molar or molecular. We assume that no variation has taken place in its rate, save that caused by deductions in moving other matter; for if its motion be supposed to have otherwise varied, the conclusion that the differences in the distances travelled result from differences in the obstructions met with, is vitiated. Thus the truth to be established is already taken for granted in the premises. Nor is the question begged in this way only. In every case where it is remarked that a body stops the sooner, the more it is obstructed by other bodies or media, the law of inertia is assumed to

hold in the obstructing bodies or media. The very conception of greater or less retardation so caused, implies the belief that there can be no retardations without proportionate retarding causes; which is itself the assumption otherwise expressed in the first law of motion.

Again, let us suppose that instead of inexact observations made on the movements occurring in daily experience, we make exact experiments on movements specially arranged to yield measured results; what is the postulate underlying every experiment? Uniform velocity is defined as motion through equal spaces in equal times. How do we measure equal times? By an instrument which can be inferred to mark equal times only if the oscillations of the pendulum are isochronous; which they can be proved to be only if the first and second laws of motion are granted. That is to say, the proposed experimental proof of the first law, assumes not only the truth of the first law, but of that which Professor Tait agrees with Newton in regarding as a second law. Is I said that the ultimate time-measure referred to is the motion of the Earth round its axis, through equal angles in equal times? Then the obvious rejoinder is that the assertion of this, similarly involves an assertion of the truth to be proved; since the undiminished rotatory movement of the Earth is itself a corollary from the first law of motion. Is it alleged that this axial movement of the Earth through equal angles in equal times, is ascertainable by reference to the stars? I answer that a developed system of Astronomy, leading through complex reasonings to the conclusion that the Earth rotates, is, in that case, supposed to be needful before there can be established a law of motion which this system of Astronomy itself postulates. For even should it be said that the Newtonian theory of the Solar System is not neces-

sarily pre-supposed, but only the Copernican; still, the proof of this assumes that a body at rest (a star being taken as such) will continue at rest; which is a part of the first law of motion, regarded by Newton as not more self-evident than the remaining part.

Not a little remarkable, indeed, is the oversight made by Professor Tait, in asserting that "no *à priori* reasoning can conduct us demonstratively to a single physical truth," when he has before him the fact that the system of physical truths constituting Newton's *Principia*, which he has joined Sir William Thomson in editing, is established by *à priori* reasoning. That there can be no change without a cause, or, in the words of Mayer, that "a force cannot become nothing, and just as little can a force be produced from nothing," is that ultimate dictum of consciousness on which all physical science rests. It is involved alike in the assertion that a body at rest will continue at rest, in the assertion that a body in motion must continue to move at the same velocity in the same line if no force acts on it, and in the assertion that any divergent motion given to it must be proportionate to the deflecting force; and it is also involved in the axiom that action and reaction are equal and opposite.

The reviewer's doctrine, in support of which he cites against me the authority of Professor Tait, illustrates in Physics that same error of the inductive philosophy which, in Metaphysics, I have pointed out elsewhere (*Principles of Psychology*, Part VII.). It is a doctrine implying that we can go on for ever asking the proof of the proof, without finally coming to any deepest cognition which is unproved and unprovable. That this is an untenable doctrine, I need not say more to show. Nor, indeed, would saying more to show it be likely to have any effect, in so far at least as the reviewer is concerned;

seeing that he thinks I am "ignorant of the very nature of the principles" of which I am speaking, and seeing that my notions of scientific reasoning "remind" him "of the Ptolemists," who argued that the heavenly bodies must move in circles because the circle is the most perfect figure.*

Not to try the reader's patience further, I will end by pointing out that, even were the reviewer's criticisms all valid, they would leave unshaken the theory he contends against. Though one of his sentences (p. 480) raises the expectation that he is about to assault, and greatly to damage, the bases of the system contained in the second part of *First Principles*, yet all those propositions which constitute the bases, he leaves, not only uninjured, but even untouched,—contenting himself with trying to show (with what success we have seen) that the fundamental one is an *à posteriori* truth and not an *à priori* truth. Against the general Doctrine of Evolution, considered as an induction from all classes of concrete

* Other examples of these amenities of controversy, in which I decline to imitate my reviewer, have already been given. What occasions he supplies me for imitation, were I minded to take advantage of them, an instance will show. Pointing out an implication of certain reasonings of mine, he suggests that it is too absurd even for me to avow explicitly; saying:—"We scarcely think that even Mr. Spencer will venture to claim as a datum of consciousness the Second Law of Motion, with its attendant complexities of component velocities, &c." Now any one who turns to Newton's *Principia*, will find that to the enunciation of the Second Law of Motion, nothing whatever is appended but an amplified re-statement—there is not even an illustration, much less a proof. And from this law, this axiom, this immediate intuition or "datum of consciousness," Newton proceeds forthwith to draw those corollaries respecting the composition of forces which underlie all dynamics. What, then, must be thought of Newton, who explicitly assumes that which the reviewer thinks it absurd to assume implicitly?

phenomena, he utters not a word; nor does he utter a word to disprove any one of those laws of the redistribution of matter and motion, by which the process of Evolution is deductively interpreted. Respecting the law of the Instability of the Homogeneous, he says no more than to quarrel with one of the illustrations. He makes no criticism on the law of the Multiplication of Effects. The law of Segregation he does not even mention. Nor does he mention the law of Equilibration. Further, he urges nothing against the statement that these general laws are severally deducible from the ultimate law of the Persistence of Force. Lastly, he does not deny the Persistence of Force; but only differs respecting the nature of our warrant for asserting it. Beyond pointing out, here a cracked brick and there a coin set askew, he merely makes a futile attempt to show that the foundation is not natural rock, but concrete.

From his objections I may, indeed, derive much satisfaction. That a competent critic, obviously anxious to do all the mischief he can, and not over-scrupulous about the means he uses, has done so little, may be taken as evidence that the fabric of conclusions attacked will not be readily overthrown.

In the *British Quarterly Review* for January, 1874, the writer of the article I have dealt with above, makes a rejoinder. It is of the kind which might have been anticipated. There are men to whom the discovery that they have done injustice is painful. After proof of having wrongly ascribed to another such a nonsensical belief as that insensible motion is heat because heat is insensible motion, some would express regret. Not so my reviewer. Having by forced interpretations debited

me with an absurdity, he makes no apology; but, with an air implying that he had all along done this, he attacks the allegation I had really made—an allegation which is at least so far from an absurdity, that he describes it only as not justified by "the present state of science." And here, having incidentally referred to this point, I may as well, before proceeding, deal with his substituted charge at the same time that I further exemplify his method. Probably most of those who see the *British Quarterly*, will be favourably impressed by the confidence of his assertion; but those who compare my statement with his travesty of it, and who compare both with some authoritative exposition, will be otherwise impressed. To his statement that I conclude "that friction must ultimately transform *all* [the italics are his] the energy of a sound into heat," I reply that it is glaringly untrue: I have named friction as a second cause. And when he pooh-poohs the effect of compression because it is "merely momentary," is he aware of the meaning of his words? Will he deny that, from first to last, during the interval of condensation, heat is being generated? Will he deny to the air the power of radiating such heat? He will not venture to do so. Take then the interval of condensation as one-thousandth of a second. I ask him to inform those whom he professes to instruct, what is the probable number of heat-waves which have escaped in this interval. Must they not be numbered by thousands of millions? In fact, by his "merely momentary," he actually assumes that what is momentary in relation to our time-measures, is momentary in relation to the escape of ethereal undulations!

Let me now proceed more systematically, and examine his rejoinder point by point. It sets out thus:—

"In the notice of Mr. Spencer's works that appeared in the last

number of this *Review*, we had occasion to point out that he held mistaken notions of the most fundamental generalizations of dynamics; that he had shown an ignorance of the nature of proof in his treatment of the Newtonian Law; that he had used phrases such as the Persistence of Force in various and inconsistent significations; and more especially that he had put forth proofs logically faulty in his endeavour to demonstrate certain physical propositions by *à priori* methods, and to show that such proofs must exist. To this article Mr. Spencer has replied in the December number of the *Fortnightly Review*. His reply leaves every one of the above positions unassailed."

In my "Replies to Criticisms," which, as it was, trespassed unduly on the pages of the *Fortnightly Review*, I singled out from his allegations which touched me personally, one that might be briefly dealt with as an example; and I stated that, passing over other personal questions, as not interesting to the general reader, I should devote the small space available to an impersonal one. Notwithstanding this, the reviewer, in the foregoing paragraph, enumerates his chief positions; asserts that I have not assailed any of them (which is untrue); and then leads his readers to the belief that I have not assailed them because they are unassailable.

Leaving this misbelief to be dealt with presently, I continue my comments on his rejoinder. After referring to the passage I have quoted from Prof. Tait's statement about physical axioms, and after indicating the nature of my criticism, the reviewer says:—

"Had Mr. Spencer, however, read the sentence that follows it, we doubt whether we should have heard aught of this quotation. It is 'Without further remark we shall give Newton's Three Laws; it being remembered that as the properties of matter might have been such as to render a totally different set of laws axiomatic, *these laws must be considered as resting on convictions drawn from observation and experiment and not on intuitive perception.*' This not only shows that the term 'axiomatic' is used in the previous sentence in a sense that does not exclude an inductive origin, but it leaves us indebted

to Mr. Spencer for the discovery of the clearest and most authoritative expression of disapproval of his views respecting the nature of the Laws of Motion."

Let us analyze this "authoritative expression." It contains several startling implications, the disclosure of which the reader will find not uninteresting. Consider, first, what is implied by framing the thought that "the properties of matter might have been such as to render a totally different set of laws axiomatic." I will not stop to make the inquiry whether matter having properties fundamentally unlike its present ones, can be conceived; though such an inquiry, leading to the conclusion that no conception of the kind is possible, would show that the proposition is merely a verbal one. It will suffice if I examine the nature of this proposition that "the properties of matter *might have been*" other than they are. Does it express an experimentally-ascertained truth? If so, I invite Prof. Tait to describe the experiments. Is it an intuition? If so, then along with doubt of an intuitive belief concerning things *as they are*, there goes confidence in an intuitive belief concerning things *as they are not*. Is it an hypothesis? If so, the implication is that a cognition of which the negation is inconceivable (for an axiom is such) may be discredited by inference from that which is not a cognition at all, but simply a supposition. Does the reviewer admit that no conclusion can have a validity greater than is possessed by its premises? or will he say that the trustworthiness of cognitions increases in proportion as they are the more inferential? Be his answer what it may, I shall take it as unquestionable that nothing concluded can have a warrant higher than that from which it is concluded, though it may have a lower. Now the elements of the proposition before us are these:—*As* "the properties of matter might have

been such as to render a totally different set of laws axiomatic" [*therefore*] "these laws [now in force] must be considered as resting . . . not on intuitive perception:" that is, the intuitions in which these laws are recognized, must not be held authoritative. Here the cognition posited as premiss, is that the properties of matter might have been other than they are; and the conclusion is that our intuitions relative to existing properties are uncertain. Hence, if this conclusion is valid, it is valid because the cognition or intuition respecting what might have been, is more trustworthy than the cognition or intuition respecting what is! Scepticism respecting the deliverances of consciousness about things as they are, is based upon faith in a deliverance of consciousness about things as they are not!

I go on to remark that this "authoritative expression of disapproval" by which I am supposed to be silenced, even were its allegation as valid as it is fallacious, would leave wholly untouched the real issue. I pointed out how Prof. Tait's denial that any physical truths could be reached *à priori*, was contradicted by his own statement respecting physical axioms. The question thus raised the reviewer evades, and substitutes another with which I have just dealt. Now I bring forward again the evaded question.

In the passage I quoted, Prof. Tait, besides speaking of physical "*axioms*," says of them that due familiarity with physical phenomena gives the power of seeing "*at once*" "their *necessary* truth." These last words, which express his conception of an axiom, express also the usual conception. An axiom is defined as a "self-evident truth," or a truth that is seen *at once;* and the definition otherwise worded is—a "truth so evident *at first sight*, that no process of reasoning or demonstra-

tion can make it plainer." Now I contend that Prof. Tait, by thus committing himself to a definition of physical axioms identical with that which is given of mathematical axioms, tacitly admits that they have the same *à priori* character; and I further contend that no such nature as that which he describes physical axioms to have, can be acquired by experiment or observation during the life of an individual. Axioms, if defined as truths of which the *necessity* is at once seen, are thereby defined as truths of which the negation is inconceivable; and the familiar contrast between them and the truths established by individual experiences, is that these last never become such that their negations are inconceivable, however multitudinous the experiences may be. Thousands of times has the sportsman heard the report that follows the flash from his gun, but still he can imagine the flash as occurring silently; and countless daily experiments on the burning of coal, leave him able to conceive coal as remaining in the fire without ignition. So that the "convictions drawn from observation and experiment" during a single life, can never acquire that character which Prof. Tait admits physical axioms to have: in other words, physical axioms cannot be derived from personal observation and experiment. Thus, otherwise applying the reviewer's words, I "doubt whether we should have heard aught of this quotation" to which he calls my attention, had he studied the matter more closely; and he "leaves us indebted to" him "for the discovery of" a passage which serves to make clearer the untenability of the doctrine he so dogmatically affirms.

I turn now to what the reviewer says concerning the special arguments I used to show that the first law of

motion cannot be proved experimentally. After a bare enunciation of my positions, he says:—

"On the utterly erroneous character of these statements we do not care to dwell, we wish simply to call our reader's attention to the conclusion arrived at. Is that a disproof of the possibility of an inductive proof? We thought that every tolerably educated man was aware that the proof of a scientific law *consisted in* showing that *by* assuming its truth, we could explain the observed phenomena."

Probably the reviewer expects his readers to conclude that he could easily dispose of the statements referred to if he tried. Among scientific men, however, this cavalier passing over of my arguments will perhaps be ascribed to another cause. I will give him my reason for saying this. Those arguments, read in proof by one of the most eminent physicists, and by a specially-honoured mathematician, had their entire concurrence; and I have since had from another mathematician, standing among the very first, such qualified agreement as is implied in saying that the first law of motion cannot be proved by terrestrial observations (which is in large measure what I undertook to show in the paragraphs which the reviewer passes over so contemptuously). But his last sentence, telling us what he thought "every tolerably educated man was aware" of, is the one which chiefly demands attention. In it he uses the word *law*—a word which, conveniently wide in meaning, suits his purpose remarkably well. But we are here speaking of physical *axioms*. The question is whether the justification of a physical axiom consists in showing that by assuming its truth, we can explain the observed phenomena. If it does, then all distinction between hypothesis and axiom disappears. Mathematical axioms, for which there is no other definition than that which Prof. Tait gives of physical axioms, must stand on the same footing. Henceforth we must

hold that our warrant for asserting that "things which are equal to the same thing are equal to one another," consists in the observed truth of the geometrical and other propositions deducible from it and the associated axioms—the *observed* truth, mind; for the fabric of deductions yields none of the required warrant until these deductions have been tested by measurement. When we have described squares on the three sides of a right-angled triangle, cut them out in paper, and, by weighing them, have found that the one on the hypothenuse balances the other two; then we have got a fact which, joined with other facts similarly ascertained, justifies us in asserting that things which are equal to the same thing are equal to one another! Even as it stands, this implication will not, I think, be readily accepted; but we shall find that its unacceptability becomes still more conspicuous when the analysis is pursued to the end.

Continuing his argument to show that the laws of motion have no *à priori* warrant, the reviewer says:—

"Mr. Spencer asserts that Newton gave no proof of the Laws of Motion. The whole of the *Principia* was the proof, and the fact that, taken as a system, these laws account for the lunar and planetary motions, is the warrant on which they chiefly rest to this day."

I have first to point out that here, as before, the reviewer escapes by raising a new issue. I did not ask what he thinks about the *Principia*, and the proof of the laws of motion by it; nor did I ask whether others at this day, hold the assertion of these laws to be justified mainly by the evidence the Solar System affords. I asked what Newton thought. The reviewer had represented the belief that the second law of motion is knowable *à priori*, as too absurd even for me openly to enunciate. I pointed out that since Newton enunciates it openly under the title of an axiom, and offers no proof whatever of it, he did explicitly what I am blamed for

doing implicitly. And thereupon I invited the reviewer to say what he thought of Newton. Instead of answering, he gives me his opinion to the effect that the laws of motion are proved true by the truth of the *Principia* deduced from them. Of this hereafter. My present purpose is to show that Newton did not say this, and gave every indication of thinking the contrary. He does not call the laws of motion "hypotheses;" he calls them "axioms." He does not say that he assumes them to be true *provisionally;* and that the warrant for accepting them as actually true, will be found in the astronomically-proved truth of the deductions. He lays them down just as mathematical axioms are laid down—posits them as truths to be accepted *à priori*, from which follow consequences which must therefore be accepted. And though the reviewer thinks this an untenable position, I am quite content to range myself with Newton in thinking it a tenable one—if, indeed, I may say so without undervaluing the reviewer's judgment. But now, having shown that the reviewer evaded the issue I raised, which it was inconvenient for him to meet, I pass to the issue he substitutes for it. I will first deal with it after the methods of ordinary logic, before dealing with it after the methods of what may be called transcendental logic.

To establish the truth of a proposition postulated, by showing that the deductions from it are true, requires that the truth of the deductions shall be shown in some way that does not directly or indirectly assume the truth of the proposition postulated. If, setting out with the axioms of Euclid, we deduce the truths that "the angle in a semicircle is a right angle," and that "the opposite angles of any quadrilateral figure described in a circle, are together equal to two right angles," and so forth; and if, because these propositions are true, we say that the

axioms are true, we are guilty of a *petitio principii*. I do not mean simply that if these various propositions are taken as true on the strength of the demonstrations given, the reasoning is circular, because the demonstrations assume the axioms; but I mean more—I mean that any supposed *experimental* proof of these propositions, by measurement, itself assumes the axioms to be justified. For even when the supposed experimental proof consists in showing that some two lines demonstrated by reason to be equal, are equal when tested in perception, the axiom that things which are equal to the same thing are equal to one another, is taken for granted. The equality of the two lines can be ascertained only by carrying from the one to the other, some measure (either a moveable marked line or the space between the points of compasses), and by assuming that the two lines are equal to one another, because they are severally equal to this measure. The ultimate truths of mathematics, then, cannot be established by any experimental proof that the deductions from them are true; since the supposed experimental proof takes them for granted. The same thing holds of ultimate physical truths. For the alleged *à posteriori* proof of these truths, has a vice exactly analogous to the vice I have just indicated. Every evidence yielded by astronomy that the axioms called "the laws of motion" are true, resolves itself into a fulfilled prevision that some celestial body or bodies, will be seen in a specified place, or in specified places, in the heavens, at some assigned time. Now the day, hour, and minute of this verifying observation, can be fixed only on the assumption that the Earth's motion in its orbit and its motion round its axis, continue undiminished. Mark, then, the parallelism. One who chose to deny that things which are equal to the same thing are equal to one another, could never have it

proved to him by showing the truth of deduced propositions; since the testing process would in every case assume that which he denied. Similarly, one who refused to admit that motion, uninterfered with, continues in the same straight line at the same velocity, could not have it proved to him by the fulfilment of an astronomical prediction; because he would say that both the spectator's position in space, and the position of the event in time, were those alleged, only if the Earth's motions of translation and rotation were undiminished, which was the very thing he called in question. Evidently such a sceptic might object that the seeming fulfilment of the prediction, say a transit of Venus, may be effected by various combinations of the changing positions of Venus, of the Earth, and of the spectator on the Earth. The appearances may occur as anticipated, though Venus is at some other place than the calculated one; provided the Earth also is at some other place, and the spectator's position on the Earth is different. And if the first law of motion is not assumed, it must be admitted that the Earth and the spectator *may* occupy these other places at the predicted time: supposing that in the absence of the first law, this predicted time can be ascertained, which it cannot. Thus the testing process inevitably begs the question.

That the perfect congruity of all astronomical observations with all deductions from "the laws of motion," gives coherence to this group of intuitions and perceptions, and so furnishes a warrant for the entire aggregate of them which it would not have were any of them at variance, is unquestionable. But it does not therefore follow that astronomical observations can furnish a test for *each individual assumption*, out of the many which are simultaneously made. I will not dwell on the fact that the process of verification assumes the validity

of the assumptions on which acts of reasoning proceed; for the reply may be that these are shown to be valid apart from astronomy. Nor will I insist that the assumptions underlying mathematical inferences, geometrical and numerical, are involved; since it may be said that these are justifiable separately by our terrestrial experiences. But, passing over all else that is taken for granted, it suffices to point out that, in making every astronomical prediction, the three laws of motion and the law of gravitation are *all* assumed; that if the first law of motion is to be held proved by the fulfilment of the prediction, it can be so only by taking for granted that the two other laws of motion and the law of gravitation are true; and that non-fulfilment of the prediction would not disprove the first law of motion, since the error might be in one or other of the three remaining assumptions. Similarly with the second law: the astronomical proof of it depends on the truth of the accompanying assumptions. So that the warrants for the assumptions A, B, C, and D, are respectively such that A, B, and C, being taken as trustworthy, prove the validity of D; D being thus proved valid, joins C, and B, in giving a character to A; and so throughout. The result is that everything comes out right if they happen to be all true; but if one of them is false, it may destroy the characters of the other three, though these are in reality exact. Clearly, then, astronomical prediction and observation can never test any one of the premises by itself. They can only justify the entire aggregate of premises, mathematical and physical, joined with the entire aggregate of reasoning processes leading from premises to conclusions.

I now recall the reviewer's "thought," uttered in his habitual manner, "that every tolerably educated man was

aware that the proof of a scientific law *consisted in* showing that *by* assuming its truth, we could explain the observed phenomena." Having from the point of view of ordinary logic dealt with this theory of proof as applied by the reviewer, I proceed to deal with it from the point of view of transcendental logic, as I have myself applied it. And here I have to charge the reviewer with either being ignorant of, or else deliberately ignoring, a cardinal doctrine of the System of Philosophy he professes to review—a doctrine set forth not in those four volumes of it which he seems never to have looked into; but in the one volume of it he has partially dealt with. For this principle which, in respect to scientific beliefs, he enunciates for my instruction, is one which, in *First Principles*, I have enunciated in respect to all beliefs whatever. In the chapter on the "Data of Philosophy," where I have inquired into the legitimacy of our modes of procedure, and where I have pointed out that there are certain ultimate conceptions without which the intellect can no more stir "than the body can stir without help of its limbs," I have inquired how their validity or invalidity is to be shown; and I have gone on to reply that—

"Those of them which are vital, or cannot be severed rom the rest without mental dissolution, must be assumed as true *provisionally* . . . leaving the assumption of their unquestionableness to be justified by the results.

"§ 40. How is it to be justified by the results? As any other assumption is justified—by ascertaining that all the conclusions deducible from it, correspond with the facts as directly observed—by showing the agreement between the experiences it leads us to anticipate, and the actual experiences. There is no mode of establishing the validity of any belief, except that of showing its entire congruity with all other beliefs."

Proceeding avowedly and rigorously on this principle,

I have next inquired what is the fundamental *process* of thought by which this congruity is to be determined, and what is the fundamental *product* of thought yielded by this process. This fundamental product I have shown to be the coexistence of subject and object; and then, describing this as a postulate to be justified by "its subsequently-proved congruity with every result of experience, direct and indirect," I have gone on to say that "the two divisions of self and not-self, are re-divisible into certain most general forms, the reality of which Science, as well as Common Sense, from moment to moment assumes." Nor is this all. Having thus assumed, *only provisionally*, this deepest of all intuitions, far transcending an axiom in self-evidence, I have, after drawing deductions occupying four volumes, deliberately gone back to the assumption (*Prin. of Psy.*, § 386). After quoting the passage in which the principle was laid down, and after reminding the reader that the deductions drawn had been found congruous with one another; I have pointed out that it still remained to ascertain whether this primordial assumption was congruous with all the deductions; and have thereupon proceeded, throughout eighteen chapters, to show the congruity. And yet having before him the volumes in which this principle is set forth with a distinctness, and acted upon with a deliberation, which I believe are nowhere exceeded, the reviewer enunciates for my benefit this principle of which he "thought that every tolerably educated man was aware"! He enunciates it as applying to limited groups of beliefs, to which it does not apply; and shuts his eyes to the fact that I have avowedly and systematically acted upon it in respect to the entire aggregate of our beliefs (axioms included) for which it furnishes the ultimate justification!

Here I must add another elucidatory statement,

which would have been needless had the reviewer read that which he criticizes. His argument proceeds throughout on the assumption that I understand *à priori* truths after the ancient manner, as truths independent of experience; and he shows this more than tacitly, where he "trusts" that he is "attacking one of the last attempts to deduce the laws of nature from our inner consciousness." Manifestly, a leading thesis of one of the works he professes to review, is entirely unknown to him—the thesis that forms of thought, and consequently the intuitions which those forms of thought involve, result entirely from the effects of experiences, organized and inherited. With the *Principles of Psychology* before him, not only does he seem unaware that it contains this doctrine, but though this doctrine, set forth in its first edition published nearly twenty years ago, has gained considerable currency, he seems never to have heard of it. The implication of this doctrine is, not that the "laws of nature" are deducible from "our inner consciousness," but that our consciousness has a pre-established correspondence with such of those laws (simple, perpetually presented, and never negatived) as have, in the course of practically-infinite ancestral experiences, registered themselves in our nervous structure. Had he taken the trouble to acquaint himself with this doctrine, he would have learned that the intuitions of axiomatic truths are regarded by me as latent in the inherited brain, just as bodily reflex actions are latent in the inherited nervous centres of a lower order; that such latent intuitions are made potentially more distinct by the greater definiteness of structure due to individual action and culture; and that thus, axiomatic truths, having a warrant entirely *à posteriori* for the race, have for the individual a warrant which, substantially *à priori*, is made complete *à posteriori*. And

he would then have learnt that as, during evolution, Thought has been moulded into increasing correspondence with Things; and as such correspondence, tolerably complete in respect of the simple, ever-present, and invariable relations, as those of space, has made considerable advance in respect of the primary dynamical relations; the assertion that the resulting intuitions are authoritative, is the assertion that the simplest uniformities of nature, as experienced throughout an immeasurable past, are better known than they are as experienced during an individual life. All which conceptions, however, being, as it seems, unheard of by the reviewer, he regards my trust in these primordial intuitions as like that of the Ptolemists in their fancies about perfection!

Thus far my chief antagonists, passive if not active, have been Prof. Tait and, by implication, Sir William Thomson, his coadjutor in the work quoted against me—men of standing, and the last of them of world-wide reputation as a mathematician and physicist. Partly because the opinions of such men demand attention, I have dealt with the questions raised at some length; and partly, also, because the origin and consequent warrant of physical axioms are questions of general and permanent interest. The reviewer, who by citing against me these authorities has gained for some of his criticisms consideration they would otherwise not deserve, I must, in respect of his other criticisms, deal with very briefly. Because, for reasons sufficiently indicated, I did not assail sundry of his statements, he has reiterated them as unassailable. I will here add no more than is needful to show how groundless is his assumption.

What the reviewer says on the metaphysical aspects of the propositions we distinguish as physical, need not

detain us long. His account of my exposition of "Ultimate Scientific Ideas," he closes by saying of me that "he is not content with less than showing that all our fundamental conceptions are inconceivable." Whether the reviewer knows what he means by an inconceivable conception, I cannot tell. It will suffice to say that I have attempted no such remarkable feat as that described. My attempt has been to show that objective activities, together with their objective forms, are inconceivable by us—that such symbolic conceptions of them as we frame, and are obliged to use, are proved, by the alternative contradictions which a final analysis of them discloses, to have no likeness to the realities. But the proposition that objective existence cannot be rendered in terms of subjective existence, the reviewer thinks adequately expressed by saying that "our fundamental conceptions" (subjective products) "are inconceivable" (cannot be framed by subjective processes)! Giving this as a sample from which may be judged his fitness for discussing these ultimate questions, I pass over his physico-metaphysical criticisms, and proceed at once to those which his special discipline may be assumed to render more worthy of attention.

Quoting a passage relative to the law that "all central forces vary inversely as the squares of the distances," he derides the assertion that "this law is not simply an empirical one, but one deducible mathematically from the relations of space—one of which the negation is inconceivable." Now whether this statement can or cannot be fully justified, it has at any rate none of that absurdity alleged by the reviewer. When he puts the question—"Whence does he [do I] get this?" he invites the suspicion that his mind is not characterized by much excursiveness. It seems never to have occurred to him that,

if rays like those of light radiate in straight lines from a centre, the number of them falling on any given area of a sphere described from that centre, will diminish as the square of the distance increases, because the surfaces of spheres vary as the squares of their radii. For, if this has occurred to him, why does he ask whence I get the inference? The inference is so simple a one as naturally to be recognized by those whose thoughts go a little beyond their lessons in geometry.* If the reviewer means to ask, whence I get the implied assumption that central forces act only in straight lines, I reply that this assumption has a warrant akin to that of Newton's first axiom, that a moving body will continue moving in a straight line unless interfered with. For that the force exerted by one centre on another should act in a curved line, implies the conception of some second force, complicating the direct effect of the first. And, even could a central force be truly conceived as acting in lines not straight, the *average* distribution of its effects upon the inner surface of the surrounding sphere, would still follow the same law. Thus, whether or not the law be accepted on *à priori* grounds, the assumed absurdity of representing it to have *à priori* grounds, is not very obvious. Respecting this statement of mine the reviewer goes on to say—

"This is a wisdom far higher than that possessed by the discoverer of the great law of attraction, who was led to consider it from no cogitations on the relations of space, but from observations of the

* That I am certainly not singular in this view, is shown to me, even while I write, by the just-issued work of Prof. Jevons on the *Principles of Science: a Treatise on Logic and Scientific Method.* In vol. ii., p. 141, Prof. Jevons remarks respecting the law of variation of the attractive force, that it "is doubtless connected at this point with the primary properties of space itself, and is so far conformable to our necessary ideas."

movements of the planets; and who was so far from rising to that clearness of view of the truth of his great discovery, which is expressed by the phrase, 'its negation is inconceivable,' that he actually abandoned it for a time, because (through an error in his estimate of the earth's diameter) it did not seem fully to account for the motion of the moon."

To the first clause in this sentence, I have simply to give a direct denial; and to assert that neither Newton's "observations of the movements of the planets" nor other such observations continued by all astronomers for all time, would yield "the great law of attraction." Contrariwise, I contend that when the reviewer says, by implication, that Newton had no antecedent hypothesis respecting the cause of the planetary motions, he (the reviewer) is not only going beyond his possible knowledge, but he is asserting that which even a rudimentary acquaintance with the process of discovery, might have shown him was impossible. Without framing, beforehand, the supposition that there was at work an attractive force varying inversely as the square of the distance, no such comparison of observations as that which led to the establishment of the theory of gravitation could have been made. On the second clause of the sentence, in which the reviewer volunteers for my benefit the information that Newton "actually abandoned" his hypothesis for a while because it did not bring out right results, I have first to tell him that, in an early number of the very periodical containing his article,* I cited this fact (using these same words) at a time when he was at school, or before he went there.† I have next to assert

* See Essay on "The Genesis of Science," in the *British Quarterly Review* for July, 1854, p. 127.

† I do not say this at random. The reviewer, who has sought rather to make known than to conceal his identity, took his degree in 1868.

that this fact is irrelevant; and that Newton, while probably seeing it to be a necessary implication of geometrical laws that central forces vary inversely as the squares of the distances, did not see it to be a necessary implication of any laws, geometrical or dynamical, that there exists a force by which the celestial bodies affect one another; and therefore doubtless saw that there was no *à priori* warrant for the doctrine of gravitation. The reviewer, however, aiming to substitute for my "confused notions" his own clear ones, wishes me to identify the proposition—Central forces vary inversely as the squares of the distances — with the proposition —There is a cosmical force which varies inversely as the squares of the distances. But I decline to identify them; and I suspect that a considerable distinction between them was recognized by Newton. Lastly, apart from all this, I have to point out that even had Newton thought the existence of an attractive force throughout space was an *à priori* truth, as well as the law of variation of such a force if it existed; he would still, naturally enough, pause before asserting this law, when he found his deductions from it did not correspond with the facts. To suppose otherwise, is to ascribe to him a rashness which no disciplined man of science could be guilty of.

See, then, the critical capacity variously exhibited in the space of a single sentence. The reviewer, quite erroneously, thinks that observations unguided by hypotheses suffice for physical discoveries. He seems unaware that, on *à priori* grounds, the law of the inverse square had been suspected as the law of some cosmical force, before Newton. He asserts, without warrant, that no such *à priori* conception preceded, in Newton's mind, his observations and calculations. He confounds the law of variation of a force, with the existence of a force varying

according to that law. And he concludes that Newton could have had no *à priori* conception of the law of variation, because he did not assert the existence of a force varying according to this law in defiance of the evidence as then presented to him!

Now that I have analyzed, with these results, the first of his criticisms, the reader will neither expect me to waste time in similarly dealing with the rest *seriatim*, nor will he wish to have his own time occupied in following the analysis. To the evidence thus furnished of the reviewer's fitness for the task he undertakes, it will suffice if I add an illustration or two of the *animus* which leads him to make grave imputations on trivial grounds, and to ignore the evidence which contradicts his interpretations.

Because I have spoken of a balanced system, like that formed by the sun and planets, as having the "peculiarity, that though the constituents of the system have relative movements, the system, as a whole, has no movement," he unhesitatingly assumes me to be unaware that in a system of bodies whose movements are not balanced, it is equally true that the centre of gravity remains constant. Ignorance of a general principle in dynamics is alleged against me solely because of this colloquial use of the word "peculiarity," where I should have used a word (and there is no word perfectly fit) free from the implication of exclusiveness. If the reviewer were to assert that arrogance is a "peculiarity" of critics; and if I were thereupon to charge him with entire ignorance of mankind, many of whom besides critics are arrogant, he would rightly say that my conclusion was a very large one to draw from so small a premise.

To this example of strained inference I will join an example of what seems like deliberate misconstruc-

tion. From one of my essays (not among the works he professes to deal with) the reviewer, to strengthen his attack, brings a strange mistake; which, even without inquiry, any fair-minded reader would see must be an oversight. A statement true of a single body acted on by a tractive force, I have inadvertently pluralized: being so possessed by another aspect of the question, as to overlook the obvious fact that with a plurality of bodies the statement became untrue. Not only, however, does the reviewer ignore various evidences furnished by the works before him, that I could not really think what I had there said, but he ignores a direct contradiction contained in the paragraph succeeding that from which he quotes. So that the case stands thus:—On two adjacent pages I have made two opposite statements, both of which I cannot be supposed to believe. One of them is right; and this the reviewer assumes I do not believe. One of them is glaringly wrong; and this the reviewer assumes I do believe. Why he made this choice no one who reads his criticism will fail to see.

Even had his judgments more authority than is given to them by his mathematical honours, this brief characterization would, I think, suffice. Perhaps already, in rebutting the assumption that I did not answer his allegations because they were unanswerable, I have ascribed to them an unmerited importance. For the rest, suggesting that their value may be measured by the value of that above dealt with as a sample, I leave them to be answered by the works they are directed against.

Here I end. The foregoing pages, while serving, I think, the more important purpose of making clearer the relations of physical axioms to physical knowledge, incidentally justify the assertion that the reviewer's charges of fallacious reasoning and ignorance of the nature of

proof, recoil on himself. When, in his confident way, he undertakes to teach me the nature of our warrant for scientific beliefs, ignoring absolutely the inquiry contained in *Principles of Psychology*, concerning the relative values of direct intuitions and reasoned conclusions, he lays himself open to a sarcasm which is sufficiently obvious. And when a certain ultimate principle of justification for our beliefs, set forth and acted upon in the *System of Synthetic Philosophy* more distinctly than in any other work, is enunciated by him for my instruction, as one which he "thought that every tolerably educated man was aware" of, his course is one for which I find no fit epithet in the vocabulary I permit myself to use. That in some cases he has shown eagerness to found charges on misinterpretations little less than deliberate, has been sufficiently shown; as also that, in other cases, his own failure to discriminate is made the ground for ascribing to me beliefs that are manifestly untenable. Save in the single case of a statement respecting collisions of bodies, made by me without the needful qualification, I am not aware of any errors he detects, except errors of oversight or those arising from imperfect expression and inadequate exposition. When he unhesitatingly puts the worst constructions on these, it cannot be because his own exactness is such that no other constructions occur to him; for he displays an unusual capacity for inadvertencies, and must have had many experiences showing him how much he might be wronged by illiberal interpretations of them. One who in twenty-three professed extracts makes fifteen mistakes—words omitted, or added, or substituted—should not need reminding how largely mere oversight may raise suspicion of something worse. One who shows his notions of accurate statement by asserting that as I substitute "persistence"

for "conservation," I therefore identify Persistence of *Force* with Conservation of *Energy*, and debits me with the resulting incongruities—one who, in pursuance of this error, confounds a special principle with the general principle it is said to imply, and thereupon describes a wider principle as being included in a narrower (p. 481)—one who speaks of our "inner consciousness" (p. 488), so asserting, by implication, that we have an outer consciousness—one who talks of an inconceivable conception; ought surely to be aware how readily lax expressions may be turned into proofs of absurd opinions. And one who, in the space of a few pages, falls into so many solecisms, ought to be vividly conscious that a whole volume thus written would furnish multitudinous statements from which a critic, moved by a spirit like his own, might evolve abundant absurdities; supplying ample occasion for blazoning the tops of pages with insulting words.

UNIVERSITY OF CALIFORNIA LIBRARY
Los Angeles
This book is DUE on the last date stamped below.

REC'D LD-URL
JUN 2 3 1983

315

UC SOUTHERN REGIONAL LIBRARY FACILITY
A 000 084 135 3